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A Stimulus in Need of a Response: A Review of *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition*

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In this commentary, I describe relational frame theory (RFT) as an analysis of complex human behavior that has been insufficiently addressed within contemporary behavior analysis. The theory is described as having an exceptionally ambitious vision of the type that will render behavior analytic accounts more generally acceptable within the broader behavioral and cognitive sciences. In my own view, inductive empirically-driven analyses derived from current data on relational learning (including my own) have not been comparably ambitious; they have not addressed the full range of phenomena to which they might be profitably applied. By contrast, researchers in the RFT tradition have ambitious; they have not addressed the full range of phenomena to which they might be profitably applied. By contrast, researchers in the RFT tradition have tended to project their analyses to encompass a variety of plausible, attractive applications that are arguably within the reach of their current data or data that may be reasonably anticipated in the future. In order for RFT researchers to have its maximum impact, however, I suggest that certain critical steps must be accomplished. First, the theory must be reconciled with the basic behavioral processes that are the core of the experimental analysis of behavior. Second, certain experiments must be conducted that have thus far not been emphasized in the RFT tradition. In particular, I suggest that the current practice of studying college students and verbal school-aged children must be supplemented with comparably intensive studies of populations with developmental limitations (e.g., typically developing children who are just acquiring language). Absent such experimentation, it seems likely that RFT will remain a plausible account that merely competes with other plausible accounts without promoting ultimate resolution of the critical issues.

Relational frame theory (hereafter RFT) represents an effort to extend behavior analytic principles (some not universally accepted) to account for many aspects of complex human behavior, including language, problem-solving, rule-governance, and a variety of other phenomena that are often included in the general area of cognition. *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition* (2001) (hereafter *RFT:ALC*) is the most ambitious, most complete presentation of RFT currently available. The book consists of thirteen chapters edited by Steven Hayes, Dermot Barnes-Holmes, and Bryan Roche. Because individual editors were also authors or co-authors of all thirteen chapters, I will henceforth refer to them as "the authors," although many of their students and colleagues contributed as well.

The essence of the theory is that contingencies of reinforcement select not only discriminative control by physical stimulus properties but also the more abstract forms of relational stimulus control that have long interested students of complex behavior (e.g., Stevens, 1951). Relational frame theory might be briefly summarized as an extension of the traditional behavior analytic account of abstraction (e.g., see Goldiamond, 1964, for an example that may be particularly relevant to the case presented in *RFT:ALC*). Readers familiar with that account will recall that abstraction may develop when a set of otherwise physically different discriminative stimuli have a physical property in common. For example, having learned via discrimination training to respond differentially to a red flower, a red car, a red ball, and other

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red items, the learner may subsequently respond differentially to other red items without the need for explicit discrimination training.

A major extension of relational frame theory has been to include within the abstraction analysis not only control by specific physical properties but also a wide variety of relational discriminations that do not depend on physical properties. For example, the relation of "dominant" may be described independently of the physical characteristics of those who dominate and those who submit; the relation is thus deemed *arbitrarily applicable* and contextually dependent. Arbitrary applicability is easy to describe in the language of the layman, but the concept is difficult to reconcile with the basic processes specified in behavior analysis—reinforcement, discrimination, response differentiation, and conditioned reinforcement (Ferster & Skinner, 1957). So too, however, is stimulus equivalence, thus leading to suggestions by Catania (1984) and Sidman (1994) that new processes must be introduced into the behavior analytic formulation to account for the growing body of empirical findings in relational learning research.

Another major departure from the abstraction analysis is that RFT views relational learning as behavior. One does not merely exhibit relational stimulus control. One "relates." By contrast, the traditional behavioral analysis of abstraction has been the narrowing of stimulus control. Thus, relational responding in RFT seems to be an active rather than a passive process. This characteristic has set many traditionally trained behavior analysts to head scratching as they tried to fit the theory within a behavior analytic framework.

I have always found relational frame theory to be most understandable as a more-or-less direct extension into the relational discrimination area of the classic analysis of generalized imitation (Baer, Peterson, & Sherman, 1967), a conceptual debt acknowledged by Hayes and colleagues in Chapter 1 of *RFT:ALC*. Baer and colleagues were faced with the empirical finding that teaching a number of specific imitative performances could result in so-called generalized imitation—the emergence of new imitative performances that had no explicit training history. How could one account for such emergent performances within an operant framework? The solution was to invoke the concept of the higher-order operant. The student had learned not only the imitative performances that had been taught explicitly but also something more. S/he had abstracted from the training that the relevant aspect of the task at hand was to behave in conformance with the behavior of the model—whatever that behavior might be. The use of the higher-order operant or similar concepts to account for complex forms of emergent behavior has always been recognizable in Skinner's thinking, particularly in *Verbal Behavior* (1957; hereafter VB).

RFT:ALC seems quite consistent with the tradition of VB. Indeed, as I read through *RFT:ALC*, I could not help comparing the challenge facing Hayes and colleagues with the similar one that faced Skinner when he was composing VB. How does one convince a general audience that behavior analysis has the necessary concepts and empirical findings to provide a comprehensive, broadly useful account of important processes involved in language and conceptually related aspects of cognition? Like VB, the authors offer us what is primarily an exercise in interpretation: Many aspects of complex human behavior are interpreted in relation to a fairly small number of behavioral principles (some specific to the theory). Unlike VB, however, the interpretive exercise is supported by a large and growing body of behavioral research with humans. This characteristic, by itself, will render the basic arguments more compelling for a general audience. In part for this reason, I found myself asking an even more challenging question: "Is behavior analysis as a field finally ready to rejoin mainstream academic debate in the areas of language and cognition?"

The Larger Debate

In framing the preceding question in the way that I have, I know that I risk alienating certain colleagues within behavior analysis. Such colleagues may make compelling arguments that if there is rejoining to be done, it ought to be the re-engagement of cognitive science with strong empirical foundation that has been built up within behavior analysis over the past 40 years. Such arguments are born of the frustration that comes with the long history of misrepresentation and misunderstanding of behavioral analysis by other subdisciplines of the behavioral sciences. However understandable this reaction may be, the fact remains that few

behavior analysts are now engaged in active interdisciplinary discourse in the general areas of language and cognition. I think that objective observers would conclude that most behavior analysts have been content to talk to other behavior analysts where such matters are concerned. These observers would likely conclude also that colleagues working in cognitive science have shown little interest in constructive engagement with behavior analysts. Most cognitive scientists continue to dismiss behavior analytic principles as at best unsophisticated or at worst obviously wrong when applied to language (e.g., Deacon, 1997).

In my own experience, few cognitive scientists feel any obligation to keep abreast of developments within behavior analysis, including those that might bear directly on their scientific interests. The same can be said about behavior analysts in relation to the cognitive sciences. Thus, a curious schism has developed. Behavior analysts and cognitive scientists study the same or similar subject matter, in some cases independently arriving at the same or similar accounts, and yet there is virtually no constructive interdisciplinary interaction (see Wilkinson, Dube, & McIlvane, 1998, for one noteworthy example). As a result, there have been few external challenges to contemporary behavior analytic thinking on language and cognition and correspondingly little influence of that thinking on the cognitive sciences.

When I ask whether our field is finally ready to re-engage with the cognitive sciences, it is with the recognition that (1) behavior analysis has long been in a position to engage mainstream academic psychology in relation to issues relating to language and cognition, and (2) cognitive science would benefit from reengagement. I concur with Sidman's (1986) suggestion that:

> Cognitive Science worries about important and interesting phenomena but has been too impatient, failing to accomplish the intellectually rigorous and prior task of laying a systematic foundation from which to synthesize complex processes. [By contrast,] Behavior Analysis ... has not been impatient enough to attempt all of the syntheses of which it is capable. An easy criticism has been that behavior analysis deals well with uninteresting behavior, but ignores everything that makes human beings [human]. The concepts of stimulus and response have seemed impoverished, unable to capture the rich complexity of the human intellect. Behavior analysts, themselves, have not continued to examine the units of their own science in sufficient depth to appreciate whether, and how, they might be able to account for just those phenomena that concern cognitive scientists. (p. 215)

In that same paper, Sidman bemoaned the lack of interdisciplinary dialogue or even apparent interest in engaging in such a dialogue. The situation Sidman described some 17 years ago has not changed much since then. A few behavioral groups, including my own, have taken recognizable steps toward addressing the interests of cognitive scientists (e.g., Griffee & Dougher, 2002; Wilkinson & McIlvane, 2001). However, such efforts have been limited and certainly do not represent a sustained field-wide engagement effort.

Into this context comes *RFT:ALC*, a book that appears to be intended to engage a broad audience of readers both within and external to behavior analysis. It seems clear that the authors have the explicit aim of fostering meaningful interdisciplinary dialogue or at least promoting discussion of interdisciplinary themes. I found the tone of the introductory section to be excellent for this purpose—an open invitation to thinkers both within and outside behavior analysis to engage with the subject matter. Given this open and appealing approach, I do not foresee *RFT:ALC* occasioning the usual complaints of some cognitive scientists about the insularity of the behavior analytic approach. Indeed, for reasons that I will develop below, I see the distinct possibility that *RFT:ALC* will receive a more positive response from broad-thinking cognitive scientists than from some conservative, traditionally trained behavior analysts.

Relational Frame Theory

While respecting the ongoing productive efforts of Hayes and his co-workers, many behavior analysts (myself included) have been frustrated with certain aspects of relational frame theory and the work that it has inspired. Speaking only for myself, I felt keenly the lack of a tight, focused presentation of the major aspects of the theory. Thus, I was particularly gratified by Chapter 8 ("Relational frame theory: A Précis"), which I found to be the

clearest, most concise presentation available thus far. While the authors encouraged us to defer reading this chapter until others were appreciated, I think that those already familiar with the theory will find it immediately helpful. I did. In fact, I would encourage Hayes and colleagues to develop possibilities for presenting this material as a target article, inviting commentary in the manner of Horne and Lowe (1996). Such an article would complement their book-length treatment and provide an opportunity to sample the general reaction of behavior analysts and other interested scientists to RFT.

Given the voluminous body of behavior analytic work on relational learning summarized in *RFT:ALC*, Horne and Lowe's article, and Sidman's (1994) book, one would expect there to have been already published a comprehensive, detailed treatment of theories of stimulus equivalence and related phenomenon. However, such a treatment has not yet appeared, and the authors chose not to attempt one in *RFT:ALC*. Paralleling one characteristic of Sidman's (1994) book, Hayes and colleagues were content to comment occasionally (and usually dispassionately) on perceived limitations of other approaches. Possible limitations of relational frame theory, however, were not much discussed in *RFT:ALC*. Rather, the authors focused on articulating relational frame theory and expanding its scope to account for a wide range of complex human behavior.

As one who has tried a little to contribute to theory-building in this general area (e.g. McIlvane, Serna, Dube, & Stromer, 2000), I was disappointed that *RFT:ALC* did not attempt to do much to energize and/or frame debate in the relational learning field. Characterizing the book as a whole, I found little effort to engage with the best of other behavior analytic thinking on the subject matter. Sidman has approached some of the same subject matter in somewhat different ways (e.g., Sidman, 1990), particularly in acknowledging that equivalence relations are not the only kinds of relations to be considered. As just one example, there is current behavior analytic work on ordinal relations (e.g., Mackay, Kotlarchyk, & Stromer, 1997) that is interpreted within Sidman's general framework. These efforts are barely mentioned, and I was disappointed that the authors passed up an opportunity to engage their colleagues in a critical area. Perhaps the most challenging and provocative aspect of the presentation appeared in Chapter 3. This key section has many noteworthy features, and in a sense represents much of the rest of the thinking presented in *RFT:ALC*. For economy of presentation, I will focus on this chapter to make some general points that apply also to several other sections of the book.

1) There is interesting, potentially important discussion of stimulus class formation as both a product and a process, including how different conceptualizations may affect the nature of research that results. These two notions of stimulus classification are often not considered separately in behavior analysis, and conceptual problems can be inadvertently created unless the distinction is maintained (cf. McIlvane & Dube, 1990). I concur with the assertion that maintaining clarity in the process vs. product distinction will help advance thinking in the relational learning arena. Lack of clarity, I believe, delayed recognition that new behavior analytic principles may be necessary to account for stimulus equivalence and other relational learning phenomena of the type described in *RFT:ALC* and elsewhere. I am not yet convinced that the notion of arbitrarily applicable relational responding is sufficiently well developed and/or articulated to serve as the new principle, but I think it is certainly a step in the right direction.

2) Another important issue identified by the authors is the need for precision in the concept of the stimulus relation in RFT. The authors clearly understand that they and the larger behavior analytic community must take care to be sure that "stimulus relation" is a tact rather than some other form of verbal behavior. In my opinion, the ultimate explanatory power of RFT or any other theory of the same subject matter will depend upon satisfactory management of this key challenge. A related challenge is to distinguish clearly whether and how there is a fundamental difference between discriminative control and contextual control. In my opinion, this remains a somewhat unsettled area in *RFT:ALC* and other current behavior analytic thinking (e.g., Sidman, 1986; Stromer, McIlvane, & Serna, 1993).

3) The concepts of complete and coherent networks seem to have potential utility, perhaps especially for communicating with cognitive scientists. One purpose of the network analysis, for example, is to provide an RFT

account of why nonsense sentences (e.g., Baffling bloated billiards behave brusquely.) can be discriminated from mere nonsense word strings (e.g., Brusquely billiards baffling bloated behaved.). Readers familiar with the behaviorist–psycholinguist debates of the past forty years will immediately recognize the contribution that well-developed concepts of this general type could make toward interdisciplinary discourse about language. I would encourage readers of *RFT:ALC* to spend some extra effort to consider the arguments presented in this section. Also, I would encourage the authors to expand on their structural analyses of grammatical frames. Given the central role of grammatical processes in the historical behavior analyst–psycholinguist debate, such analyes seem extremely important and merit substantially more development than was attempted in *RFT:ALC*.

4) The authors make a good case for the need to expand and/or elaborate the historical concept of a stimulus class. Related to this is the distinction in relational frame theory between transfer and transformation of stimulus functions. The distinction may be especially hard for conservative behavior analysts to accept, particularly those who think of stimulus classes as defined by the common responses they occasion (e.g., Goldiamond, 1966).

The notion that behavioral functions can transform rather than merely transfer via stimulus class formation appears challenging on its face. In my own thinking, I have always found it helpful to draw parallels between transformation of function and the concepts of behavioral resurgence, spontaneous interconnection of repertoires, and other similar phenomena (e.g., Epstein, 1985). In this regard, I was somewhat surprised to see so little discussion of these behavioral phenomena in *RFT:ALC*.

One problem that I had with *RFT:ALC*'s discussions of transformation of functions was the general lack of an obvious relationship to the basic processes of behavior analysis that were mentioned earlier. Abstract concepts can best aid thinking and discussion when one can unambiguously relate a given behavioral phenomenon to well-defined operations and processes. By contrast, transformation of function seems itself to be a behavioral process as described in *RFT:ALC*, yet it is neither defined as an irreducible behavioral primitive nor explicitly defined in terms of other constituent processes. I found this omission to be an unsatisfying solution to a conceptually difficult problem. Sidman's assertion that equivalence relations are a basic behavioral primitive was in part an effort to deal directly with the problem that was avoided here.

In my opinion, the terms "behavioral process," "behavioral primitive," and "behavioral function" are often not precisely defined in contemporary work on relational learning. I have always understood a behavioral primitive to refer to a behavioral process that could not be reduced to other processes. A behavioral function, by contrast, seems to be a somewhat different type of term. In my own thinking, the function of an operant is found in its consequences; a stimulus may function, for example, as an eliciting stimulus, a discriminative stimulus, and/or a conditioned reinforcer. In RFT, I sometimes find it difficult to relate "stimulus functions," "behavioral functions," and so on to well-defined functions such as those listed above. More clarity along these lines would be helpful for me and I suspect many other readers.

5) The authors noted an important point of convergence between relational frame theory and Sidman's analysis—the notion that exposure to contingencies of reinforcement is essential in the development of contextual stimulus control. This convergence point cannot be overemphasized in my opinion. The main difference between Sidman's thinking and that articulated in *RFT:ALC* is whether complex forms of stimulus control are selected by or constructed via reinforcement contingencies. Once the selectionist vs. constructivist issue is ultimately settled, I anticipate fairly few points of major disagreement between Sidman's followers and the thinking represented in *RFT:ALC*.

6) There were several topics in Chapter 3 and elsewhere in which I would have preferred a more informed and/or more self-critical presentation. For example, stimulus generalization is described as a generally agreed upon behavioral process; the presentation ignores the longstanding debate about whether generalization is a process or a product of other processes (Prokasy & Hall, 1963).

Another example was their discussion of the relationship between stimulus familiarity/history contingency interactions, relational responding, and "common sense." The authors

passed up an important opportunity to address a common criticism of much of the empirical work inspired by RFT—inadequate experimental control of the pre-experimental behavioral history/repertoire. As background to developing this point further, the authors point out that complex relational networks can be extremely difficult to understand when described using the notation that is common among equivalence researchers (e.g., "For participants in Study 1, A1 is equivalent to B1 except in the context of higher-order stimulus RB"). They contrast that situation with the ease with which such networks are understood in the context of stimuli and stimulus relations that are familiar to groups of typical university students in the U.S. or Europe (e.g., "For speakers of English, 'cat' is equivalent with a picture of a feline except in Spanish class").

The preceding contrast was made to point out that while the two types of networks are formally equivalent, the latter seem less challenging from a common sense perspective. In other words, common sense suggests that networks of familiar, meaningful stimuli are less arbitrary—clearly fallacious reasoning of the type that bothers behavior analysts when it appears in the literature of psycholinguistics. Behavior analysts have always argued that common sense thinking is the product of the shared contingencies of a verbal community—any verbal community will do. As just one of many possible examples, consider the response of non-English-speaking Chinese graduate students of formal logic to the "arbitrary" and "conventional" networks of the type described by the authors and illustrated above; clearly the Chinese logicians would prefer the "easy" abstractly presented networks to the more difficult ones involving European languages. That probable outcome would be totally consistent with RFT and core behavior analytic principles.

The opportunity passed up in *RFT:ALC* is to discuss the limitations of empirical studies that take advantage of extra-experimentally established relational performances. The vast majority of studies summarized in *RFT:ALC* were conducted with university students. Predominant use of these populations of convenience has always bothered me. In my opinion, the most important challenge to RFT is to demonstrate the operant contingencies that lead to arbitrarily applicable relational responding. By focusing on university students, the opportunity is lost to understand how such responding develops in the first place. Such students would not have been admitted to the university without first proving themselves capable of relational performances that greatly exceed in complexity anything that has thus far been addressed in RFT research. Indeed, readers of Chapter 9 ("Psychological Development") may wonder with me why so little RFT research addresses the processes of behavior development rather than outcomes. Put another way, when behaviorally sophisticated research populations are emphasized, it is not possible to know the degree to which the behavioral data are due to intra-experimental vs. the extra-experimental contingencies.

It is critical that the reader understand that I am not arguing that nothing can be learned by examining developmental outcomes of behaviorally sophisticated research populations. Clearly, much data of interest can be collected from such populations. However, it strikes me that general emphasis on behaviorally sophisticated populations leaves RFT theorists in a weaker position than they might otherwise be. Only studies that emphasize behavioral development can ultimately confirm RFT as opposed to merely providing behavioral data are consistent with the theory. Indeed, certain developing populations would seem particularly attractive for this purpose (see below).

CONCLUSIONS

To conclude this review, I will take an opportunity to make some general comments and to ask some questions about relational frame theory that predate the publication of *RFT:ALC*. I share the conviction with Hayes and colleagues that behavior analysis has the potential to account for many aspects of complex human behavior. Although the speculative analyses offered in the later chapters of *RFT:ALC* are likely to make conservative behavior analysts uncomfortable, these authors should be commended for challenging our field to engage with important subject matter that we have tended to avoid in the past. In turn, I think it appropriate to challenge RFT-inspired researchers to engage with subject matter that makes them somewhat uncomfortable. To do

so, I will pose a few questions meant to occasion some further reflection and discussion.

1) Why has relational frame theory not inspired a systematic replication of the widely cited study by Devany, Hayes, and Nelson (1986)? That study has been interpreted as showing that verbal but not nonverbal intellectually disabled individuals display equivalence—clearly providing major data in support of RFT theory and thus in need of replication. Other than a study with children who had severe hearing impairments that produced ambiguous findings (Barnes, McCullagh, & Keenan, 1990), no one seems to have followed up on this work. Has this population lost its attractiveness to RFT researchers? As a researcher interested in problems of persons with intellectual disability, I appreciate the need to clarify the important issues that were raised in that study (e.g., Carr, Wilkinson, Blackman, & McIlvane, 2000).

2) Following up on an earlier point, why have RFT researchers paid so little experimental attention to longitudinal studies of typically developing infants? This would seem to be the most critical test population for relational frame theory. Here, aspects of the theory could be tested directly rather than merely hypothesized or inferred. Such experimental work is clearly within the grasp of current behavioral science methods (e.g., Kagan, 1981; Lipkens, Hayes, & Hayes, 1993), and it is curious that relational frame research emphasizes other populations, typically verbally sophisticated children and adults.

3) Why do relational frame theorists not more directly consider arguments that behavior that may be the product of phylogenic contingencies (cf. Skinner, 1969)? For readers unfamiliar with the subject matter, Sidman (1994) and others have argued that stimulus equivalence and related phenomena may be traceable to such contingencies rather than the ontogenic contingencies. Avoidance of the phylogenic contingency issue is not problematic if one has a compelling logical analysis and/or empirical data that lead one to discount it. Unfortunately, neither is currently available.

As just one illustrative case where phylogenic contingency analysis may be needed, consider generalized imitation, a prototypical example of a higher-order operant. As noted earlier, it is widely believed that generalized imitation emerges from training many exemplars. What is not widely appreciated is that teaching multiple exemplars fails to establish generalized imitation in a substantial number of individuals (e.g., Guess, Sailor, & Baer, 1978). Thus, the multiple exemplar account at best represents an incomplete analysis. Might understanding possible effects of phylogenic contingencies help us more fully understand this important phenomenon?

I have no compelling evidence supporting an ontogenic–phylogenic contingency intersection analysis. It is based merely on (a) the assumption that species with a phylogenically selected imitative capacity would have a profound reproductive advantage over nonimitative species and (b) the fact that higher primates and perhaps other species have some imitative capacity (e.g., Whiten & Custance, 1996) which is difficult to trace to multiple exemplar training. Thus, I think it entirely possible that imitation training succeeds in producing generalized performance when it engages phylogenically selected behavioral capacities and fails when abnormal neurological development interferes with those capacities.

In making the preceding argument, I recognize that much more development would be needed to provide a phylogenic-ontogenic contingency interaction theory of generalized imitation. Offering such an account is not my intention here. My point is merely that one can construct phylogenic-contingency adjuncts or alternatives to the *exclusive* ontogenic contingency analyses presented in *RFT:ALC*. I think a more active debate of these issues within behavior analysis would be a healthy development, paralleling a similar ongoing discussions within psycholinguistics (e.g., Bates et al., 1998).

The questions that I have just posed are intended mainly as constructive criticism and food for thought. Indeed, it would be disingenuous on my part to criticize excessively the effort represented by *RFT:ALC*. Anyone can find fault and/or areas of disagreement with one's colleagues thinking. I find myself in full agreement with the ambitious nature of the effort. It would be too easy for conservative behavior analysts to deal RFT the death of 1,000 cuts by pointing out sections of *RFT:ALC* that are inadequately supported empirically and/or overreach intellectually. Worse still would be to ignore the effort. Without question, relational frame theory as laid out in *RFT:ALC* is an en-

gaging stimulus that demands a more detailed, more articulate response than has been offered thus far.

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